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DISCUSSION PAPER SERIES

6 – 2012/1

**“DEAR PROFESSOR MANKIW ...” – REFLECTIONS ON FOUR
DECADES OF LEARNING AND TEACHING
*MACROECONOMICS***

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JANUARY 2012

* The immediate inspiration for this paper are the open letter to Mankiw on November 2, 2012 by the ‘Concerned Students of Economics 10’ (**Harvard Political Review**, downloaded at: <http://hpronline.org/harvard/an-open-letter-to-greg-mankiw/> on 18 January, 2012) and the thoughtful reflection on it, in the context of *macroeconomic* curricula at selected Indian Universities by Visakh Varma in his recent article in EPW (Varma, 2012). For the convenience of the interested reader, the ‘open letter to Mankiw’ is added as an appendix to the main text of this paper.

“For economics[♥], or rather that part of it which from time to time claims a monopoly of defining the subject, has always been victim of history. For lengthy periods, when the world economy appears to be rolling on quite happily with or without advice, history encourages a good deal of self-satisfaction. Proper economics has the floor, improper economics is tacitly excluded, or consigned to the twilight world of past and present heterodoxy, the equivalent of faith-healing or acupuncture in medicine. However, *from time to time history catches economists at their brilliant gymnastics and walks off with their overcoats*. The early 1930s were such a period, and we are living through another such. At least some economists are dissatisfied with the state of their subject.”

Eric Hobsbawm: **Marshall Lecture I**, Faculty of Economics & Politics, Cambridge University, 1980, Reprinted in Hobsbawm (1997, Chapter 7, p. 95; italics added)

[♥] Obviously Hobsbawm is referring, here, to *Macroeconomics*.

§ 1. A Personal and Professional Preamble

“So I got up and asked:

Is it your view that if I went out and bought a new *overcoat*, that would increase unemployment?

‘Yes’, said Hayek. ‘But’, pointing to his triangles on the board, ‘it would take a very long mathematical argument to explain why.’”¹

Richard Kahn (1984), p. 180; italics added.

Half a century separated the *Marshall Lectures* given by Eric Hobsbawm in 1980 and Hayek’s talk to the *Marshall Society* in 1931. Ostensibly, the only common element in their two fascinating lectures, each – of course – vastly so in widely differing ways, were *overcoats*!

I began studying – and almost immediately also teaching – macroeconomics exactly forty years ago, the very year that could be said to have initiated the *Lucasian*, or *Newclassical*, Macroeconomic ‘Revolution’. *Expectations and the Neutrality of Money* (Lucas, 1972), the *Lucasian* fountainhead of what is sometimes referred to as the *Rational Expectations Revolution in Macroeconomics*, ‘witheringly rejected’ by the Journal to which it was first submitted (Lucas, 1981, p. 10), was also published forty years ago. In this sense, I have lived all my ‘macroeconomic life’ in the dark shadows cast by the rise – and rise – of Newclassical economics², spending my time in trying to ‘catch them in their brilliant gymnastics’ and to walk ‘with their [mathematical] overcoats’.

Mercifully, the macroeconomics I was taught, and I hope I learned, at the University of Lund, in Sweden and Cambridge University, in the 1970s, emphasized the *Wicksellian tradition* at the former (by Björn Thalberg, a student of Frisch, Haavelmo, Eric Lundberg and Bent Hansen) and the *tradition of Keynes*³ at the latter (by Nicky Kaldor, Richard Goodwin, Joan Robinson, Mario Nuti and Luigi Pasinetti, students, friends and contemporaries of Keynes, Sraffa, Kalecki, Schumpeter and Harrod). These traditions have left indelible marks in the way I have approached the teaching of regular courses in macroeconomics, at both undergraduate and various postgraduate levels at Universities in Asia, Europe, Latin America and the US⁴.

¹ The full context of this quote is clearly set out in Richard Kahn’s elegant *Raffaele Mattioli Lectures* (Kahn, 1984, pp. 179-180), delivered in the year of the **Keynes Birth Centennial** (1983). The formal institutionalization of The Marshall Lectureship was only achieved the year after Hayek’s talk at the Marshall Society, with – I believe – the first of the *Marshall Lectures* eventually given by Jacob Viner in the academic year 1946-7.

² In 1971 the Nobel Memorial Prize in Economics was awarded to Simon Kuznets who was one of the earliest critics of Equilibrium Non-Endogenous Business Cycle Theory and an acknowledged pioneer of national income accounting and the fruitful role descriptive statistics can play in applied economics. Forty years later, the Prize was shared by two who are the antithesis of what I may call the Kuznets philosophy, methodology and epistemology. Forty years ago next year, the same prize was awarded to Wassily Leontief. Who else but Lance Taylor could be considered a ‘modern’ Leontief (& Stone)?

³ *Not*, I must emphasise, the *Keynesians*.

⁴ Over the past forty years I have taught Macroeconomics at the University of Lund, Cambridge University, the European University Institute, Aalborg University, the People’s University in Beijing, UCLA, the Central

In addition to these formal modes of learning macroeconomics, there were also the unspoken, informal, traditions I learned from long, deep and fruitful friendships with John Hicks, Geoff Harcourt, Bob Clower, Richard Day, John McCall and Lance Taylor, all of which left deep and powerful ways of *viewing* and tackling the classic macroeconomic pathologies and *interpreting* orthodoxies critically.

Professor Visakh Varma has raised issues in the choice of curricula for the teaching of macroeconomics, presumably at the postgraduate level, admittedly in an Indian context, that have preoccupied my own approach to effective pedagogy of a subject famous for being a servant of events than a master of them, except occasionally. In this note, my thoughts on the issues he has perceptively raised, are outlined.

The paper is structured as follows: in the next section a succinct summary of the ‘vision’ – in a special Schumpeterian sense – with which I set out in my macroeconomic journeys is outlined; in section 3 the orthodox approaches are outlined; section 4 contains notes on four alternative visions of macroeconomics, each an alternative challenge to one or another aspect of orthodoxy. Finally, I try to extract the lessons I have learned from adhering to the alternative visions, but without adopting them uncritically.

Macro was never taught as if all issues have been resolved – both conceptually and theoretical technologically; always scope for new macroeconomic pathologies; always ‘armed’ with tools, concepts and a framework with which to work, adapt and adjust – especially with the way intersectoral international balances are arrayed; balance sheets are made to evolve as financial transactions take new forms; to learn the basic repertoire of a useful mathematical formalism, so that new techniques can be learned and adapted.

§ 2. Macroeconomic Visions

“In practice we all start our own research from the work of our predecessors, that is, we hardly ever start from scratch. Obviously, in order to be able to posit to ourselves any problem at all, we should first have to visualize a distinct set of coherent phenomena as a worth-while object of our analytic efforts. In other words, analytic effort is of necessity preceded by a preanalytic cognitive act that supplies the raw material for the analytic effort. In this book, **this preanalytic cognitive act will be called Vision**. It is interesting to note that vision of this kind not only must precede historically the emergence of analytic effort in any field but also may re-enter the history of every established science **each time somebody teaches us to see things in a light of which the source is not to be found in the facts, methods, and results of the pre-existing state of the science.**”

Joseph Schumpeter, 1954, p. 41; bold emphasis added.

My learning, research and teaching on macroeconomics, starting from my predecessors, in the above Schumpeterian senses, taught me to see the macroeconomic pathologies of monetary maladjustments, aggregate fluctuations, policy conundrums, trade imbalances and growth in terms of challenging, consistently and systematically, the assumption of *Say's Law of Markets* (both national and international), the *efficient market hypothesis*, efficiency of equilibria, the underpinning of macroeconomic policy in the two fundamental theorems of welfare economics, the assumption of *Olympian rationality* (Simon, 1983, p. 19) for individuals behaving as expected utility maximizers, the irrelevance of *equilibrium theorizing*, especially in macrodynamics, and the relentless extolling of the virtues of the mathematization of economics, as a virtue in itself.

Simultaneously, we were taught to emphasise the notions of effective demand and involuntary unemployment in the context of the *fallacy of composition*, via the *Banana Parable* (Keynes, 1930, p. 178), the *Widow's Cruse* to highlight the significance of functional distribution and its interaction with growth, so-called fundamental uncertainty (but not necessarily in formal probabilistic terms⁵), the tradition of *Political Arithmetic* on which to base and understand the social and national accounts as a basis for the discussion of monetary imbalances (both national and international), the philosophy and methodology of what Joan Robinson, in her *Inaugural Lecture* (Robinson, 1966) called *The New Mercantilism*, essentially a sustained critique of the theory of comparative advantage, the crucial significance of the *stock-flow divide*⁶, especially in monetary macroeconomic dynamics, and, thus, to the controversies over liquidity preference vs. loanable funds in determining the level of the money rate of interest and the pervasive non-neutrality of money and the follies of the quantity theory of money. To these were, of course, added the distinctive Goodwinian themes of *endogenous*, nonlinear, evolutionary, dynamic *vision* of the growth cycle dynamics of macroeconomic fluctuations, both real and monetary, Joan Robinson's sustained critical vision of *capital theory*⁷ and the characteristic

⁵ None of my Cambridge teachers either formalized any of their macrodynamics in terms of any kind of probabilistic underpinning, nor even remotely envisaged the usefulness of the expected utility maximization framework for the Olympian rationality of the individual agent to be made a part of monetary macroeconomic dynamics. The lesson I learned was that either nonlinearity or incompleteness – not uncertainty – or some judicious combination of the two, in parsimonious models was more than sufficient to understand, represent and remedy macroeconomic pathologies. Ditto for econometrics, of the traditional variety, and, instead, a great deal of emphasis was made on the necessity, desirability and feasibility of *numerical simulations* to understand, iterate between theoretical specifications – even of the balance sheet and national accounts variety – and prediction for policy purposes.

⁶ Kalecki's characteristically succinct, yet pungent view on this was reported by Joan Robinson (Robinson, 1982 in her mercilessly critical review of Leijonhufvud (1981):

“When Michal Kalecki was in London, soon after the publication of Keynes' *General Theory*, Richard Kahn and I had a date to meet him at a restaurant. We arrived first, and as Michal came over to the table where we were sitting he announced ‘I have found out what economics is; it is the science of confusing stocks with flows’. It is this confusion that has kept the Quantity Theory of Money alive until today. ...

Professor Leijonhufvud has never understood Kalecki's point so that his monetary theory is both confused and confusing.”

⁷ Professor Varma's insightful remarks on Gautam Mathur's efforts at Osmania University (*op.cit.*, p. 24), at introducing Cambridge *Macrodynamics* and its integration with the sectoral analysis of Sraffa, Leontief and von

Kaldorian emphasis on increasing returns to scale, coupled to the old Smithian themes of ‘the division of labour and the extent of the market’.

There were only a handful of *macroeconomic classics* we were encouraged – even admonished – to read and become familiar with: **Wicksell’s** *Interest and Prices* & Vol. I of his *Lectures*, **Schumpeter’s** *Theory of Economic Development*, **Robertson’s** *Banking Policy and the Price Level*, the *Treatise on Money* (volumes I & II) by **Keynes**, *Monetary Equilibrium* by **Myrdal**, *The General Theory* by **Keynes**, *Studies in the Theory of Economic Expansion* by **Lundberg**, **Kalecki’s** *Studies in the Theory of Business Cycles*, *Studies in the Theory of Expansion* by **Lundberg** and **Lindahl’s** *Studies in the Theory of Money and Capital*⁸. Neither **Fisher**, nor **Hayek**, were even mentioned, even non-felicitously, especially not in macroeconomic contexts, and ditto for *Value and Capital* by **Hicks**.

That these modern macroeconomic classics stood on the mighty shoulders of the works of the *Classical Economists* was never forgotten; nor did **Marx**, **Hilferding** and **Rosa Luxembour** go unmentioned, felicitously⁹. Lectures by Dobb, Goodwin, Kaldor, Nuti, Pasinetti and Joan Robinson, invoked the names of the classical economists, and Keynes, as if they were intimate friends – and we were expected to understand the contexts, without further ado!

As for the *theoretical technology*¹⁰ (Lucas, 1981, p. 9) of monetary macrodynamics, at least as far as I was concerned, my main teachers were Björn Thalberg at Lund and Richard Goodwin at Cambridge, and both emphasized *nonlinear dynamics*, the particular vision of optimal aggregate growth, coupled

Neumann reminds me that both **Planning for Steady Growth** by Mathur (1965) and Sukhamoy Chakravarty’s **Capital and Development Planning (Chakravarty, 1969)**, as well as works by K.N. Raj, were frequently referred to by Joan Robinson, Maurice Dobb and Richard Goodwin, in the Cambridge of the early 1970s. In particular in connection with capital and (optimal) growth theory, in relation to planning for development. During my brief visit to JNU a couple of years ago, Professor Prabhat Patnaik’s recollection of a typically pungent observation by Gautam Mathur should be mentioned here. During his sojourn at Nuffield College in Oxford, in the early 1960s, at the height of the *Cambridge Controversies in Capital Theory*, Mathur, observing some of the Oxford neoclassical stalwarts and their inflexible ‘High Table’ manners, remarked: ‘These neoclassicals are cavalier about the malleability of capital but refuse to countenance the enjoyment of a fine meat dish with fish knives.’

⁸ When Kaldor interviewed me, in May, 1973, just before admitting me as a PhD student at King’s College, Cambridge, indeed, to be supervised by him, the only question he asked me was: ‘Why do you want to come here when you have Lindahl in Sweden?’ I could not, of course, remind him that Lindahl had died in 1960. I came to Cambridge, having completed a Master’s degree in Political Economy at the University of Lund (my M.Sc thesis was supervised by Professor Björn Thalberg).

⁹ When Richard Goodwin agreed to take me on as his student and when I told him about my interests in capital theory and endogenous cycle theory, his main response was: ‘Any economics student who wants to work seriously on these theories must read Marx and Böhm-Bawerk’.

¹⁰ Meaning by this phrase, essentially, mathematical and numerical techniques, to serve the needs of the economic theorist as a modeler, to tell his or her story in one of many persuasive ways and to illustrate the subtle nature of reason’s limits in drawing conclusions, especially, on the policy front. That economics was not applied mathematics, even if it was not made too clear whether it was a moral science, belonged to the humanities or exclusively a social science, was always a backdrop to whatever was the focus of discussion at any one point in time.

to an ethical grounding of policy, coming down the Ramsey line – but not in the way orthodoxy has appropriated it - and the importance of transaction matrices and the balance sheets. The latter was, for Thalberg, the Oslo tradition of Ragnar Frisch and Leif Johansen; for Goodwin, it was the Leontief-Stone tradition, not unrelated to the way Stone re-interpreted and generalized *The Multiplier as Matrix* (Goodwin, 1949) to integrate the national income accounting implicit in *How to Pay for the War* (Keynes, 1940) and the intersectoral accounting of Sraffa and Leontief, which led to the remarkably fertile work of Pyatt and Roe (1977)¹¹.

The orthodox triptych of equilibrium, rationality and optimization was systematically challenged by focusing on evolutionary disequilibria, varieties of bounded rationality and satisficing – although the ‘pretty, polite, techniques’ that mathematized the former was, we were cautioned, not easily available for the formalization of the latter triptych. The subtle message here was that the bright and the audacious could explore new pathways of unorthodox research in challenging orthodoxy in building foundations for macrodynamics on this alternative triptych. We, as students, were encouraged to learn from Shackle and Simon, Nelson and Winter, Nikaido and (Jacob) Schwartz¹² and, of course, Sraffa and von Neumann. Taming the unruly dynamics of stock-flow interactions by means of disciplined accounting, studying the disequilibrium dynamics of aggregate fluctuations in employment and output, understanding the mechanisms that underpinned functional distribution of income, managing the monetary dynamics of production economies and, above all, making sure that the tools and concepts that were devised – that were constantly in need of renewal – served the needs of compassionate visions that did not exclusively focus on the apologetics of equity-efficiency tradeoffs.

On reflection now, forty years later, it seems to me that the implicit emphasis on the theoretical technology of learning from simulation and, therefore, modeling monetary-production macrodynamic systems – whether aggregative, as in Robinson, Kaldor and Goodwin, or disaggregative, as in Sraffa, Stone and Godley – in ways that made it possible to do so, was a hallmark of *the epistemology of Cambridge macroeconomics*. Orthodoxy caught up with this epistemology, by fits and starts, via the move from Arrow-Debreu General Equilibrium (ADGE) to Computable General Equilibrium (CGE) and Applied Computable General Equilibrium (ADGE) to its – hopefully – final graveyard in

¹¹ One day, in early 1976, Richard Goodwin told me that Richard Stone had called him and asked him, bluntly: ‘Whatever happened to the *Multiplier as Matrix*?’. The reason was that Stone was preparing the *Foreword* to the work by Pyatt and Roe on constructing *Social Accounting Matrices* for planning development in Sri Lanka, and wanted to link this work with his classic work with Champernowne and Meade (Stone, et. al., 1942), when they were constructing national income accounting schemes to implement Keynes’ suggestion in *How to Pay for the War*. This is the *Political Arithmetic* of Petty (Stone, 1980), coming down from Keynes and the Swedish and Oslo tradition of national income and balance sheet tradition of Lindahl, Frisch and Leif Johansen, now nobly being developed by Lance Taylor (see also Hicks, 1956).

¹² I can recall with much vividness that the leading bookshop in Cambridge in the early 1970s regularly stocked the two volumes of *Lectures on the Analytical Method in Economics* by Jacob Schwartz.

Stochastic Dynamic General Equilibrium (SDGE), mediated by the Real Business Cycle (RBC) claims of generalizing ADGE with Recursive Competitive Equilibrium (RCE) to dynamise the static CGE. This is a path towards infusing orthodox macroeconomics with an epistemology of computation, but without sacrificing the altar of equilibrium. The virtue of Cambridge and Oslo was to have been there, at the beginning, without any obeisance to any kind of equilibrium, but respecting the *balances* of the intrinsic dynamics of accounting.

Cambridge, on the other hand, did not ever abandon the Marshallian heritage of reasoning and persuading with geometry – curve-sketching, even in Macroeconomics – of which Joan Robinson¹³ and Richard Goodwin¹⁴ were supreme exponents (Kaldor less so), as much as Keynes, Robertson, Sraffa and Dobb were masters of exquisite prose. Pedagogy vied equally with analysis – Pigou’s ‘tool-makers’ and ‘tool-users’ – made famous in the credo popularized by Joan Robinson in the opening sentences of her classic on **Imperfect Competition** (ibid, p.1):

“This book is presented to the analytical economist as a box of tools. It is an essay in the technique of economic analysis, and can make only an indirect contribution to our knowledge of the actual world. It is only by using their tools upon observed facts that economists can build up that working model of the actual world which it is their aim to construct. To tinker with the tool-box is merely a preliminary to the main attack The gap between the tool-makers and the tool-users is a distressingly wide one, and no economists can fail to have sympathy with the impatience of the politician, the business man, and the statistical investigator, who complain of the extremely poor,, arid, or even misleading information with which the analytical economists provide him.”

That the tools must be fashioned to serve the purposes of the problems to be solved was the message; not that the problems must be adapted for the use of the tools, as it has become in orthodoxy, particularly in its newclassical variants (see the next section). Stigler’s critical observation, in his perceptive review of Samuelson’s **Foundations** (Stigler, 1948, p. 605; italics added), was an implicit warning to us, as students, and was highlighted in the Cambridge pedagogy that I experienced:

“Some of the infinities of mathematical possibilities are discussed, but only in the most formal terms; there is no instance of the enlargement of our knowledge of economic processes in our society. Samuelson may reply that he is only providing tools, but *who can know what tools we need unless he knows the material on which they will be used.*”

Thus, I left my formal education period in macroeconomics with a vision of the importance, even the superiority of, the theoretical technologies of *curve-sketching* and *numerical simulation* to study the consistency of monetary accounts – quite apart from learning the importance of the mathematics of

¹³ There are 83 diagrams in **The Economics of Imperfect Competition** (Robinson, 1933), and less than half that number in that classic of *persuasion by geometry*, the **Principles** by Marshall (1924)! *Contrariwise*, as Alice may have said, there is not a single diagram in the main text of **The Accumulation of Capital** (Robinson, 1956); they are ‘relegated’ to a special section, after the main text, titled *Diagrams* (of which there are only 19 in the 3rd edition of a book that is over 450 pages in length!

¹⁴ There are 66 diagrams in **Elementary Economics from the Higher Standpoint** (Goodwin, 1970), in a book of only 199 pages; Robinson (op.cit) is 348 pages long and Marshall’s **Principles** (8th edition) is a staggering 858 pages in length! The ratio of geometry (and mathematics, but it was the *geometry* of nonlinear dynamics) to words in any writing by Goodwin was always very high, and taking notes during his lectures was a veritable nightmare.

nonlinear dynamical systems. Much of the era of newclassical macroeconomics has been dominated by diametrically opposed theoretical technologies, but – strangely – it is orthodoxy that is trying to catch up with what Cambridge was practicing, and preaching, at least epistemologically, throughout the era dominated by Keynes and his immediate followers.

§ 3. Evolution of Varieties of Orthodoxy

[T]he meaning of the word *macroeconomics* has changed to refer to the *tools being used* rather than just to the study of business cycle fluctuations.

Edward C. Prescott (2004), p. 371; italics added.

If to this remarkably jaundiced view of what macroeconomics is supposed to have *become* one adds the convictions of another of the stalwarts of newclassical macroeconomics¹⁵, then one may be forgiven for thinking that the old distinction Pigou made famous between ‘tool-makers’ and tool-users’ has been obliterated:

“[A]s economic analysts we are directed by, if not *prisoners of, the mathematical tools* we possess.”
Sargent (1987, p. xix; italics added).

The unspoken message seems to be that the ‘tool-users’ – Joan Robinson’s *economic analyst* – are interchangeable with the ‘tool-makers’. Central bankers and finance ministers – even Prime Ministers, witness the examples of India and Italy, to take two obvious current examples - are, therefore, trained in ‘tool-making’, first, and become ‘tool-users’ in their incarnation as applied economic analysts. No wonder, then, that policy making reflects – contrary to those celebrated final words by Keynes in the **GT**¹⁶ – ‘the newest ideas’ emerging from both *fresh-water* and *salt-water* departments of economics (Hall, 1976, p.1).

Robert Gordon (1989;2004), however, felt able to pronounce¹⁷, if not an obituary for newclassical economics, at least a decline of ‘the original Lucas version of new-classical macroeconomics’ characterized by *continuous market clearing* (Say’s Law), *rational expectations* and *imperfect*

¹⁵ Indeed the most recent Economics Nobel Laureate, whose early opinion quoted above, is echoed in the philosophy, methodology and epistemology enunciated by the founding father of newclassical macroeconomics, Lucas (1980; 1981), in his even less enlightened *Methodology of Business Cycles* and, of course, codified by Prescott’s above summarizing statement on ‘the meaning of the word Macroeconomics’, in his Nobel Prize Lecture, given on December, 8, 2004.

¹⁶ Most economists of my generation, or earlier, would have committed to memory that famous Keynesian reflection (**GT**, p. 384): “... the ideas which civil servants and politicians and even agitators apply to current events are not likely to be the newest.”

¹⁷ Indeed, even the zenith point of newclassical economics’ ‘short-lived period of peak influence’, is dated with admirable precision (ibid, p. 228, footnote 2): ‘The high-water mark can be placed fairly precisely at 8.50 A.M. EDT on Friday, October 13 1978, at Bald Peak, New Hampshire, just before Robert Barro and Mark Rush began their presentation of an empirical test of the policy-ineffectiveness proposition ...’ Alas, these obituaries have not reached the ears of those in Stockholm who determine the winners of the annual Nobel memorial award to economists – or, perhaps, it is a time-lag that is at work? Sargent and Sims must read their ‘obituaries’ with some amusement!

information – thus obliterating the distinction between risk and uncertainty that once united Cambridge (Keynes) and Chicago (Knight). All he needed to add to this new triumvirate was the fourth feature of *intertemporal optimization* to completely characterize the SDGE finessing of the original neoclassical closure: preferences, endowments and technology which, in turn, is manifested in the classic triptych of rationality, equilibrium and optimality (or efficiency).

In the pre-dawn, halcyon period of a homogeneous macroeconomic orthodoxy, just about the time I began attending graduate classes in the subject, there was almost no controversy in the choice of Samuelson's **Foundations** and Patinkin's **Money, Interest and Prices** (Patinkin, 1965) as the core textbooks. The former provided the tools that were harnessed by the latter to make the neoclassical synthesis pedagogically transmittable to generations of graduate students. True, Clower's rumblings, challenging the implicit claims of a consistency between Walrasian microeconomics and Keynesian macroeconomics was becoming an increasing irritant.

However, the full force of orthodoxy's belief in intertemporally optimal, continuously cleared markets, peopled by imperfectly informed, yet rationally expecting agents, populated in islands, providing microeconomic foundations for macroeconomic, was yet to be codified by an alternative macroeconomics. Some would date its conception with Friedman's celebrated address to the AEA in 1967; others, like me, prefer to date it, not from conception, but from actual birth: forty years ago with *Expectations and the Neutrality of Money* (Lucas, 1972). Monetary neutrality, hence the quantity theory of money, the endogenising of labour supply, bringing with it the theoretical technology of recursive macroeconomics – *Markov Decision Processes*, *Dynamic Programming* and *Kalman Filtering* – and the enhancing of a dubious aggregate production function with the additional argument of human capital, Lucas set in motion, in one fell swoop, a fully-fledged macroeconomic revolution that has matured into what at the frontiers of the subject is now referred to as the SDGE model.

The path from the neoclassical synthesis to orthodoxy's current theoretical and applied frontiers, with SDGE modeling and so-called *business cycle accounting* (Chari, et.al., 2002), is easy, with hindsight, to trace. But before such a potted history is outlined, it might be useful to record, for the benefit of 'posterity', the actual genesis of the term neoclassical synthesis. In his Perugia Lectures, Clower (1973; italics added) recalled a conversation with Paul Samuelson:

"Do you remember anything about the events of 1951-2 in the U.S? The Korean War, yes, but something else that is rather more important. McCarthy. Who was he attacking? Samuelson among others. There were a lot of people attacking him as a communist and a left winger and a Keynesian and so forth. I asked him once, after Axel Leijonhufvud's book came out whether he had read it and he said yes, he had started reading it but he had thrown it down after chapter 2 because he regarded this as an unwarranted attack which simply indicated that it was not worth doing serious theory. I tried to convince him that there

was more to it than that, in fact I said “I really think you should go on and read what he has to say more seriously about the *neo-classical synthesis*” and Samuelson’s response was: “*What could he say about that, it is just a term to describe the way in which one can think about these problems to avoid conflict with either the left or the right*”. Of course I, like everyone else at that time, had taken it for granted for several years that there was some kind of analytical content in the neo-classical synthesis. “*Oh, not at all*”, said he, “don’t you remember what was happening in those years?” And I said “no”. “Well, *McCarthy was after me and I put in the neo-classical synthesis and suggested that it was just a matter of point of view in order to get him off my back*”.

From these pseudo-scientific origins, the neoclassical synthesis, at least in its policy-oriented and textbook versions, came to be complemented – and ‘completed’ if a formal sense – with the incorporation of the Phillips trade-off, thus putting to rest many Patinkinian ghosts on the *classical dichotomy*. Independently, of course, another kind of ‘completion’ was achieved via the work of Arrow and Debreu (ADGE)¹⁸, which is the fountainhead for the path towards SDGE, via Scarf’s powerful research program on CGE¹⁹, leading to varieties of policy-oriented AGE models, even encompassing general equilibrium modelling of development, and finding its final (hopefully) resting place in the recursive competitive equilibrium of RBC theorists. With developments in endogenous growth theory integrated into this latter framework, underpinned by the two fundamental theorems of welfare economics, it was only a matter of time before all this was synthesised (*sic!*) as the SDGE model²⁰.

The feeble, entirely ad hoc, attempt to resurrect some semblance of a path from the neoclassical synthesis to what is, at the frontiers of macroeconomics, called New Keynesian economics – essentially incorporating varieties of ‘stickiness’ in adjustment and market dynamics, as well as less-than-Olympian rational behaviour by optimising agents²¹ - suffered, in my opinion a just and stillborn

¹⁸ As Foley (2004, p. 191) perceptively observed:

“Nobody [at MIT during Foley’s time there], curiously enough, talked to me about the Cambridge [capital] controversy at the time. Later, I became interested in the topic and wanted to know what people at MIT had been thinking. I asked Karl Shell about it, and he said, as far as he remembered, Solow and Samuelson viewed the Cambridge, England, position in the capital controversy as a lesser threat to their ambitions for neoclassical theory than the work of Gerard Debreu. I think that is interesting, and the more I think about it, the more I think it has a core of truth.

¹⁹ Incidentally, Hahn’s Inaugural Lecture at Cambridge, given on 28 February 1973, titled *On the Notion of Equilibrium in Economics* (Hahn, 1973), had nothing to say about this line of development of ADGE (nor anything, indeed, about what was to become neoclassical macroeconomics even though Prescott was a seminar speaker at Churchill College, Hahn’s citadel at Cambridge, in 1973), although I remember his scathing remarks on Brouwer’s constructivism, mentioned felicitously in Scarf (1973), during the graduate lectures by him that I attended in 1973/4.

²⁰ None of the computability or constructivity claims of CGE, AGE, RCE, RBC or SDGE are correct, as I have shown in a series of papers (see, Velupillai (2006, 2009). Samuelson’s (1998, p. 35; italics in the original) wise remark on this kind of calisthenics is worth recalling:

““The phoenix of *real* business cycles has been whistled up anew. But it has not come from the ashes of a wrongly discarded real business cycle methodology. That, like herpes, has always been with us.

What is new, and a little foolish, is the concept of a Pareto-optimal real business cycle.”

²¹ Leading, in its turn, to the kind of *Behavioural Macroeconomics* with a New Keynesian slant, most closely associated with the work of Akerlof (see Akerlof, 2001 & Akerlof and Shiller, 2009). Mario Nuti’s elegant

death, *in one of its incarnations*, as *The New Neoclassical Synthesis and the Role of Monetary Policy* (Goodfriend & King, 1997).

But, of course, it arose, like a hydra-head, in another incarnation, as Woodford's **Interest & Prices**²², whereby, we were now told that the long-run world was newclassical, but there was room for living and working in the short-run with 'Keynesian' features, contemptuously dismissed by hard-core Lucasians who, of course would not countenance any ground to Keynes, bastardised or not.

Two kinds of codification of orthodox macroeconomics have to be remembered by those of us who, holding alternative visions, have to teach the subject. I shall, for simplicity, refer to them as the New Keynesian and the SDGE credos – the former culled out of John Taylor (2000) and the latter from Romer (1989). The SDGE credo for macroeconomic modelling, codified by Romer, amounts to the following:

1. Growth is a general equilibrium process; a growth theorist must construct a *dynamic general equilibrium model* underpinned by explicit specifications of preferences, technology and an equilibrium concept.
2. The mathematical tool to be used in the characterization of dynamic *competitive* equilibrium models **should be** the Kuhn-Tucker theorem since it offers a **procedure** for reducing the problem of *calculating* competitive equilibria to that of *solving* a maximization problem²³.
3. Of all the **policy questions concerning growth**, the most fundamental is whether there are any policies that an *omniscient, omnipotent, benevolent social planner* could implement to raise the welfare of all individuals in an economy; i.e., in formal terms, the question is **whether or not equilibria are Pareto optimal**.
4. To treat this question seriously, economists must generate a set of models with Pareto optimal & Pareto suboptimal equilibria, such that policy questions w.r.t growth facts can be reduced to **a choice** from such a set.
5. Given the equivalence between saddle points and competitive equilibria, the economic theoretical implications of the Kuhn-Tucker theorem are:
 - i. The sufficient conditions of the theorem embody the **First Fundamental Theorem of Welfare Economics**: i.e., **competitive equilibria are Pareto optimal**

critique of this line of research is something to which I subscribe wholeheartedly, as is evident from my own series of published works on this topic (Nuti, 2009, Velupillai, 2010).

²² Although third rate macroeconomists, several in my own immediate academic environment, feel they can aspire to a higher status by choosing to title their pathetic papers by paraphrasing the great macroeconomists – paraphrasing, for example, *The Economic Consequences* of this or that – it was most surprising for me, anyway, to find that a serious scholar like Woodford using the title from Wicksell's classic! Mercifully, the subtitles are quite different. Wicksell's is a characteristically humble, *A Study of the Causes Regulating the Value of Money*; Woodford's the more ambitious, *Foundations of a Theory of Monetary Policy*.

²³ These claims are formally and demonstrably false.

- ii. The necessary conditions of the theorem imply the *Second Fundamental Theorem of Welfare Economics*: i.e., for any Pareto Optimally determined quantities, there exists a price system that decentralizes these quantities as a competitive equilibrium

The John Taylor credo (Taylor, 2000), which codifies the underpinnings of the contents of a standard, very successful, highly pedagogical, graduate text such as David Romer's **Advanced Macroeconomics** (Romer, 2006), is as follows. In teaching macro, and – I presume – practicing it as a policy-oriented economist, ‘it is useful to emphasize five key components of macroeconomics’ (*ibid.*, p. 90):

“First, the long-run real GDP trend, or potential GDP, can be understood using the growth model developed by Robert Solow and that has now been extended to make ‘technology’ explicitly endogenous. Second, there is no long-run trade-off between inflation and unemployment, so that monetary policy affects inflation but is otherwise neutral with respect to real variables in the long run. Third, there is a short-run trade-off between inflation and unemployment with significant implications for economic fluctuations around the trend of potential GDP; the trade-off is due largely to temporarily sticky prices and wages. Fourth, expectations of inflation and of future policy decisions are endogenous and quantitatively significant. Fifth, monetary-policy decisions are best thought of as rules, or reaction functions, in which the short-term nominal interest rate (the instrument of policy) is adjusted in reaction to economic events.”

To this must be added that ‘Ponzi schemes’ are ruled out by decree (Romer, 2006, p. 52) and, therefore, macroeconomic pathologies, like the ones under which we are now living, are impossible to encapsulate in any form – for understanding or even prediction purposes²⁴.

Nothing in either of these credos about effective demand or increasing returns to scale (except the sleight of hand with which something that is supposed to be an element of increasing returns to scale is incorporated in endogenous growth models); nothing, of course, of the fallacy of composition nor of the Widow’s Cruse; not the slightest hint of endogenous, nonlinear, dynamics and the intrinsic non-separability of growth and cycles; the quantity theory and monetary neutrality rules; growth, by decree, is optimal and efficient; policy is legal only in terms of the fundamental theorems of welfare economics, and thereby, officially underpinned by pseudo-rigorous microeconomics. Obviously, in the long-run – I am, of course, tempted to invoke Keynes – Say’s Law also rules. And so on.

Intertemporally efficient, rationally expected equilibria, are the rule; even if, from time to time, the representative agent is replaced by an overlapping generations model, with which some of the many possibilities for discretionary policy when endogenous nonlinearity underpins the intertemporal structure of the dynamic interaction between different generations of coexisting, rational agents.

²⁴ No more elegant description of this kind of somnambulant approach to macroeconomic modeling than Keynes’ acid reflection is necessary (Keynes, 1937; 1973, p. 114):

“All these pretty, polite, techniques, made for a well-panelled board room and a nicely regulated market, are liable to collapse [when ‘the forces of disillusion ... suddenly impose a new conventional basis of valuation’].”

And so, what kind of textbook adventures could I offer my graduate students? What are the current substitutes for what was, in my time as a graduate student, the staples: the **Foundations** by Samuelson and **Money, Interest, and Prices** by Patinkin (1965) – neither of which taught me optimal stochastic control theory, Markov decision processes, Kalman – or any other – filtering theory, not even dynamic programming? The former is now replaced by **Recursive Methods in Economic Dynamics** (Stokey & Lucas, 1994); the latter by **Interest and Prices** (Woodford, 2000), supplemented by Romer (2006). Although this has been the staple diet, for that part of my macroeconomic course which tries to familiarize the students with the core contents of orthodoxy, Sargent’s series of three advanced textbooks on macroeconomics – **Macroeconomic Theory** (Sargent, 1978; 1987), **Dynamic Macroeconomic Theory** (1987) and **Recursive Macroeconomic Theory** (Ljungqvist & Sargent, 2000; 2004) – are also utilized, partly to tell the student of the story of the development of newclassical macroeconomics, on their own terms²⁵.

What this means, for someone like me with radically alternative visions and commanding entirely different theoretical technologies, is that I have to retool myself constantly, even while pursuing my own agenda in tool-making²⁶, in mathematical methods that I know are irrelevant for economics, especially since every frontier mathematical framework is seriously deficient in numerical meaning – for computing, simulating and for any other algorithmic purpose.

§ 4. Four Alternative Macroeconomic Traditions

“Yet if the only form of *tradition*, of handing down, consisted in following the ways of the immediate generation before us in a blind or timid adherence to its successes, “*tradition*” should positively be discouraged. We have seen many such simple currents soon lost in the sand; . . . *Tradition* is a matter of much wider significance. It cannot be inherited, and if you want it you must obtain it by great labour. It involves, in the first place, the historical sense”

From: *Tradition and the Individual Talent* by T.S. Eliot

I began an essay on *Alternative Macroeconomics*, more than two years ago, as a summary of the visions provided by five recent – and not-so-recent – monographs²⁷ on non-traditional approach to

²⁵ Lest the unsuspecting reader thinks I am trying to give the impression that I was teaching superhuman students, who were able to read, with complete mastery of the theoretical technologies that these texts demanded, then let me assure them that this was – and is – not so. The idea has always been to recommend one or some of them as sub-core textbooks, selected chapters on which I lecture, contextually, and always against the background of the non-traditional content in one of the main texts (usually Taylor, 2004 and, now, also Taylor 2010: in earlier years also Flaschel, et. al., 1997) and Chiarella & Flaschel, 2000).

²⁶ In my case computability theory, constructive analysis and the nonlinear dynamics part of dynamical systems theory, from the strictly technical part, leaving aside epistemological and philosophical studies to make sense of these theoretical technologies in economic contexts.

²⁷ Lance Taylor (2004), Godley & Lavoie (2007), Frydman & Goldberg (2007), Patnaik (2008) and Skidelsky (2009).

macroeconomic theory²⁸, in my attempt at extracting a unified theme from these five approaches – all with common themes, but each emphasising different, important, aspects neglected by the twin-orthodoxies of current dominancy. My *Alternative Macroeconomics* was subtitled: *Rekindling Keynes* (Taylor, 2004, 2010) & Skidelsky, 2009), *Revising Hayek* (Frydman & Goldberg, 2007, 2011), *Reinterpreting Marx* (Patnaik, 2008) & *Rejuvenating Wicksell* (in parts, Godley & Lavoie, 2007). When I was nearing completion of a first draft of that essay, Lance Taylor’s supremely attractive, interestingly provocative, thoroughly enjoyable **Maynard’s Revenge** (Taylor, 2010) appeared on the ‘scene’, together with Frydman & Goldberg’s (2011) elegant **Beyond Mechanical Markets**. The former is a less technical than **Reconstructing Macroeconomics**, but more copiously endowed with institutional and doctrine-historical context; the latter, equally, a less demanding, updating, of **Imperfect Knowledge Economics**.

Whereas in my previous graduate course in macroeconomics, given last year, I had used Taylor (2004) and Frydman & Goldberg (2007) as the core textbooks, supplemented by selected chapters of Romer (2006) and Ljungqvist & Sargent (2000;2004), this time (a course just completed), I decided to combine **Maynard’s Revenge** and **Beyond Mechanical Markets**, with Romer (2006) and Sargent (1987) – the latter chosen explicitly to contrast its handling of ‘*Monetary Economics and Government Finance*’ with the much more historically, institutionally and doctrine-historically enlightened approach in Taylor’s recent book.

All of the non-traditional texts mentioned above emphasise, *in varying degrees*, all those aspects of an interesting *monetary* macroeconomic theory that does not rule the possible emergence of aggregate pathologies: of inflation, unemployment, policy conundrums, the fallacy of composition, Widow’s cruse issues of functional income distribution coupled to the dynamics of class conflict, social and national accounting highlighting intrinsic stock-flow dynamics, endogenously fluctuating growth dynamics modeled with the theoretical technology of nonlinear mathematics, occasionally also the interactions between social and national accounting consistency and aggregate endogenous dynamics, the irrelevance of Say’s Law and its financial market handmaiden – the efficient market hypothesis, the mendacity of assuming universal individual rationality and rational expectations, the role of imperfect knowledge and fundamental uncertainty, the importance of increasing returns to scale, the crucial role of effective demand, also in its incarnation as Marx’s realization problem, asset price bubbles understood from several different perspectives, and much else.

²⁸ By ‘non-traditional’ I mean, of course – and also meant by the authors of the five excellent books mentioned above – neither the dominant *Newclassical* nor the limping *New Keynesian* approaches to ‘macroeconomic theory’.

But none of these interesting, enlightened, challenging and anti-orthodox texts, potentially – and in actual fact, at least in my hands - core textbooks in graduate monetary macroeconomic courses, can be considered Wicksellian, in the sense in which Lindahl, Myrdal, Hammarskjöld and Lundberg, extended and generalized **Interest and Prices** in the direction of monetary disequilibrium dynamics. Although Wicksell took as his starting point, for his macroeconomic monetary analysis, a stationary (equilibrium), from which the famous cumulative processes emerged, the Wicksellians started from the fact that ‘the very existence of a monetary mechanism implies dynamic conditions’ and, therefore, ‘the best method’ [to understand and explain] the dynamics of a monetary production economy was ‘to take a state of disequilibrium as a starting point of [their] analysis’ (Rosenstein-Rodan, 1936; see also Timlin, 1942). The Wicksellians were uncompromising in juxtaposing dynamics with disequilibrium in monetary production economies; but above all, they did not restrict the dynamics to be that which was tamable by one or another kind of differential or difference equation²⁹.

That Wicksell and the Wicksellians developed a monetary theory, developing an essentially Cambridge cash-balance theory of money to introduce a wedge between orthodoxy’s eternal division between just consumption and investment – whether atemporal or intertemporal – a third element, money-saving, is not overemphasized. Instead, the traditional division between the loanable funds-liquidity preference theories of the determination of the rate of interest has dominated the tendency to highlight the non-Keynesian aspect of the Wicksellians monetary macroeconomics.

As Mabel Timlin pointed out, in what I think is the earliest – and easily the most clear and comprehensive - monograph on **Keynesian Economics** (Timlin, 1942, p. 4):

“The consequences of this innovation in the theory of money for trade-cycle theory were quickly perceived; yet so solidly established was the concept of a self-equilibrating universe, the ‘automatic’ repercussions of which tended toward a mathematically determined norm, that terms were borrowed from the vocabulary of religion to describe the attitude of economists toward this tenet of their philosophies. Those who subscribed to the dogma were ‘orthodox’. Any man who challenged the doctrine could be almost as effectively damned before his fellow economists by being called a ‘heretic’ as any dissenter from the dogmas of the medieval church could be.”

That the Wicksellians incorporated effective demand is also clear, albeit in a convoluted way, by Lindahl’s reinterpretation and finessing of Wicksell’s notion of ‘all goods’ to a consideration of only ‘consumption goods’ made it possible for Myrdal and Lindahl to point out that the demand for consumer goods – i.e., effective demand – is that part of national income that is not saved. This gave them a direct path to national income accounting, in which they were far advanced to Keynes and his Cambridge followers, who came to it after **How to Pay for the War** (Keynes, 1940).

²⁹ Solow (1990, p. 36) noted, with characteristic perspicacity:

“God made many more stories than differential equations, so one should not pass too easily from formal similarity to story-telling.”

I have always maintained that the Wicksellians were acutely aware of this and, therefore, refused to countenance a straitjacketing of the dynamics of their monetary macroeconomic disequilibrium dynamic models with attractors of one or another type.

Wicksellian monetary macroeconomics was as richly characterized by the social and national income accounting discipline of stock-flow analysis and its intrinsic dynamics³⁰, the dismissal of Say's Law of markets, the importance of the fallacy of composition (see Lundberg, 1996, esp. p. 31, ff & Myrdal, 1982, p. 167³¹), the significance of Widow's Cruse distribution dynamics (Chiodi & Velupillai, 1983), the crucial role played by fundamental uncertainty, itself linked to *ex ante*–*ex post* analysis and, hence, also to stock-flow consistent modelling, justice in taxation (Wicksell, 1896 and Lindahl, 1919) and, hence, the role of equity in the devising of policy (Myrdal, 1934) – but also of disequilibrium, unstable, dynamics.

Thus, I am able – for pedagogical purposes – to indulge in an immanent critique – in the sense of Myrdal (1939)³² - of Taylor (2004, 2010), and thereby try to give content to a Wicksellian Monetary Macroeconomics almost on an equal footing with **Maynard's Revenge**. From the point of view of teaching, I have found the method of immanent criticism, to build a bridge between two traditions, most satisfyingly successful.

§ 5. Visions Beyond Traditions

“ For most of us, this is the aim
 Never here to be realised;
 Who are only undefeated
 Because we have gone on trying ...”

From: *Dry Salvages* by T.S. Eliot

The cardinal difference between the non-traditional alternative visions of macroeconomics, theoretically and from a policy-oriented point of view, and the orthodox approach is the uncompromising monetary foundations upon which the former is built and the real, traditional microeconomic basis on which the latter is founded. Given this difference, particularly from Marxian, Wicksellian and Keynesian points of view³³, dynamics and disequilibria are pervasive. The

³⁰ As Hicks pointed out, in the **Lindahl Festschrift** (Hicks, 1956, p. 141):

“The vital discovery which made possible the analysis of a process of change, in properly economic terms, was the introduction of accounting procedure. While economists were fumbling around to find a set of categories by which they could make a formal analysis of economic change, other people were doing the job in a professional manner. *In all its main forms, modern economic dynamics is an accounting theory.* ...[I]t is in accordance with this that social accounting should be its main practical instrument of application.”

³¹ During personal conversations with me, in 1981, Myrdal told me how he tried to explain the ‘Banana parable’ to Jacob Viner, in Geneva in 1930/31, but failed miserably!

³² Myrdal clarified the methodology he was adopting, in **Monetary Equilibrium** (*ibid*, p.v; italics added)

“Rather than pioneer with a wholly new approach, it was quite natural for the present author to project his own ideas within Wicksell's old framework. Indeed this mode of presentation has been carried so far in the present essay that it constitutes an *'immanent criticism'* of Wicksell. Although the method of immanent criticism reveals certain disadvantages when compared with a direct attack on the problems of monetary theory, its use is justified here *because it makes possible the presentation of Wicksell's theory in modern dress.*”

³³ And it will be no incongruence of any sort in including Schumpeter in this trilogy of pioneers.

macroeconomy is not an underlying static or stationary entity which is bombarded, exogenously, by *ad hoc shockeries*³⁴ for the dynamics to emerge.

In teaching, and even more importantly in research, to be confronted with a field that is ‘complete’ is distressing. It is hard, if not impossible, to inspire students, particularly the good and interested ones, to decide to choose such a subject for further exploration and, eventually, as their research field from which to embark on their doctoral studies. Mercifully, non-traditional macroeconomics is never a ‘complete’ subject, from any point of view, whereas its orthodox step-cousin is often presented as open-ended only from the point of view of theoretical technologies.

Three visions beyond the traditions are emphasized in my lectures on macroeconomics, to challenge the interested and audacious students to think about framing non-traditional monetary issues in empirically meaningful ways that have policy relevance. Having spent much time in devising, and explaining the construction of, accounting schemes and modeling their intrinsic *dynamics* – in line with Taylor (2004, 2010) – and indulging in a great deal of *curve-drawing*³⁵, I then go on to point out, first via the following perceptive observation by Maury Osborne (1977, p. 34; italics added), that economic and financial data are, at best, rational numbers:

“As for the question of replacing rows of closely spaced dots by solid lines, you can do that too if you want to, and the governors of the exchange and the community of brokers and dealers who make markets will bless you. *If you think in terms of solid lines while the practice is in terms of dots and little steps up and down*, this misbelief on your part is worth, I would say conservatively, to the governors of the exchange, at least eighty million dollars per year.”

In this age of almost universal familiarity with the digital computer, especially by graduate students (even those who may not be too advanced in formal mathematical ability or knowledge), the implications of this important observation by Osborne has significant consequences for the way economic data is considered. We – as teachers – are cavalier about the structure of the data that is generated by any entity of the economic system, assuming that they are a realization from the real number field. However, no one in their right minds would assume anything but natural numbers – at best rational numbers – for the tables that underpin national income and product accounts or the balance sheets, again at any level of aggregation and any frequency of observation.

³⁴ Richard Day’s felicitous characterization of newclassical business cycle methodology (Day, 1992).

³⁵ In recent months I have been trying to get to the roots of the tradition of the ‘graphical method of curves’ (Jenkin, 1870; 1996), made routine by Jevons and Marshall for the Anglo-Saxons (and presumably by Cournot for the ‘Continental Europeans’). I believe this to be as mendacious a practice as indiscriminate mathematization of economics. Jenkin’s reference to the ‘graphical method’ relies on the traditional graph theorem of orthodox real analysis and goes back to the Dirichlet-Kuratowski definition of the function concept. Such a definition should be replaced by the *recursion theoretic graph theorem* (Odifreddi, 1989, pp. 135-137), if studies of data are to be pursued on the screen of a digital computer.

I then outline the standard graph theorem of ordinary real analysis – which is known to every student of economics, at least implicitly, since all of them, without exception draw the demand-supply curves in microeconomics, the IS-LM diagram at the elementary macroeconomics level, and so on. It is then easy to convince them that the digital computer, at any finite resolution, cannot represent continuous data – even if such were available in the economic domain – and ask them whether it is possible to modify the standard graph theorem so that it is applicable to the mathematics of the computer. Very few – if any at all, so far, in over ten years of posing this question, I have not encountered a single graduate student who is able to make sense of this question – are able to answer in any sensible way.

At this point I present them with the recursion theoretic graph theorem, which implies that the data characterizing it has to be generated from some kind of recursive or recursively enumerable set, defining and explaining all of these terms ‘geometrically’ and intuitively.

Gradually, the students are taught to study the available data – from any source – assuming they are being generated from recursive or recursively enumerable data sets. The first step having been achieved, the next step is to disabuse them of thinking that data is generated by a probability mechanism.

Finally, the really difficult pedagogical problem of trying to disabuse the students of automatically modeling dynamics in terms of standard dynamical systems theory – at the most elementary level, in terms of simple differential or difference questions. Once the students, almost always without any exceptions, are comfortable with the trivial and realistic fact that all realized economic and financial data come as rational or natural numbers, or integers, and begin to become familiar with the assumption that they have to be generated from sets of numbers that are recursive or recursively enumerable, if they are to be depicted on digital computers with finite precision resolution, it is easy to convince them that the dynamics should be generated by mapping such numbers into themselves: i.e., rational or natural number dynamics.

This gives content to Solow’s perceptive remark that God may have made more stories than we can tell with differential equations! These stories by God, beyond the power of dynamical systems to encapsulate in any formalism, should be enriched by Paul Samuelson’s *story*, in his *Nobel Prize Lecture* (Samuelson, 1970; see also Nuti, 2009), reminding those of us who think some of the stories that an interesting – i.e., a nonlinear – multiplier-accelerator model of aggregate fluctuations can encapsulate, cannot, in any formal way, be squared with a ‘maximum dynamical system’. If so, how can we re-tell these stories and try to learn from them? This is where the essential Wicksellian belief

and practice of *simulation* takes on a life of its own, even at the level of elementary monetary macroeconomics.

None of my students, in all my years of teaching macroeconomics, whether orthodox or non-traditional, has ever found it difficult to accept God's or Samuelson's stories, and that they can tell more than any formalism or cannot be reconciled with maximization stories, respectively.

Just before I 'rest my case', I remind the students of Sraffa's deep and enduring comment, at the **Corfu Conference on Capital Theory**, reacting to Hicks on *The Measurement of Capital in relation to the Measurement of Other Economic Aggregates* (Sraffa, 1961, p. 305)³⁶:

“[O]ne should emphasize the distinction between two types of measurement. First, there was the one in which the statisticians were mainly interested. Second there was measurement in theory. The statisticians' measures were only approximate and provided a suitable field for work in solving index number problems. The theoretical measures required absolute precision. Any imperfections in these theoretical measures were not merely upsetting, but knocked down the whole theoretical basis.”

The point here is that nothing of the 'theoretical basis' of any of the alternative visions of macroeconomics needs to be knocked down, because all of them are compatible with the way actual data is generated by the existing economic system. This is not the case for the orthodox alternatives.

³⁶ But I also remind them of an example of ignorance and arrogance by quoting from Prescott (2005, p. 523: italics added):

“In the 1960s there was the famous Cambridge capital controversy. *This controversy bears on the issue 'What is money?'* The Cambridge capital controversy was a *silly one*, as pointed out so clearly by Arrow ... Arrow being a general equilibrium theorist, pointed out that there are multiple types of capital goods and with multiple capital goods only under very special conditions is there an aggregate capital stock. I emphasize that this does not mean that a model with a single capital good, which is matched to the value of some capital stock statistic, is not useful in drawing scientific inference.”

No finer example of the thorough confusion between *measurement in theory* and *statistical measurement* is easier to find, than these *silly* (sic!) assertions and claims.

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HARVARD POLITICAL REVIEW

[Harvard](#) — November 2, 2011 2:23 am

[An Open Letter to Greg Mankiw](#)

By [Harvard Talks Politics](#)

The following letter was sent to Greg Mankiw by the organizers of today's [Economics 10 walkout](#).
Wednesday November 2, 2011

Dear Professor Mankiw—

Today, we are walking out of your class, Economics 10, in order to express our discontent with the bias inherent in this introductory economics course. We are deeply concerned about the way that this bias affects students, the University, and our greater society.

As Harvard undergraduates, we enrolled in Economics 10 hoping to gain a broad and introductory foundation of economic theory that would assist us in our various intellectual pursuits and diverse disciplines, which range from Economics, to Government, to Environmental Sciences and Public Policy, and beyond. Instead, we found a course that espouses a specific—and limited—view of economics that we believe perpetuates problematic and inefficient systems of economic inequality in our society today.

A legitimate academic study of economics must include a critical discussion of both the benefits and flaws of different economic simplifying models. As your class does not include primary sources and rarely features articles from academic journals, we have very little access to alternative approaches to economics. There is no justification for presenting Adam Smith's economic theories as more fundamental or basic than, for example, Keynesian theory.

Care in presenting an unbiased perspective on economics is particularly important for an introductory course of 700 students that nominally provides a sound foundation for further study in economics. Many Harvard students do not have the ability to opt out of Economics 10. This class is required for Economics and Environmental Science and Public Policy concentrators, while Social Studies concentrators must take an introductory economics course—and the only other eligible class, Professor Steven Marglin's class Critical Perspectives on Economics, is only offered every other year (and not this year). Many other students simply desire an analytic understanding of economics as part of a quality liberal arts education. Furthermore, Economics 10 makes it difficult for subsequent economics courses to teach effectively as it offers only one heavily skewed perspective rather than a solid grounding on which other courses can expand. Students should not be expected to avoid this class—or the whole discipline of economics—as a method of expressing discontent.

Harvard graduates play major roles in the financial institutions and in shaping public policy around the world. If Harvard fails to equip its students with a broad and critical understanding of economics, their actions are likely to harm the global financial system. The last five years of economic turmoil have been proof enough of this.

We are walking out today to join a Boston-wide march protesting the corporatization of higher education as part of the global Occupy movement. Since the biased nature of Economics 10 contributes to and symbolizes the increasing economic inequality in America, we are walking out of your class today both to protest your

inadequate discussion of basic economic theory and to lend our support to a movement that is changing American discourse on economic injustice. Professor Mankiw, we ask that you take our concerns and our walk-out seriously.

Sincerely,
Concerned students of Economics 10

Downloaded on 18 January 2011 at: <http://hpronline.org/harvard/an-open-letter-to-greg-mankiw/>